studies of his University: "To explain difficulties in these questions, the atmospheric strata have been shuffled in accordance with laboratory experiments." Thus, for example, the mean pressure of the atmosphere remains, on the average of the whole year, 0·038 inch lower for every 100 miles we proceed north in this country, a difference which is called a *gradient*, as if it were a fall on a railway line, though it is really the position of equilibrium like that of the watery ocean, which has also a gradient of nearly thirty miles from the equator to the poles.

I have previously remarked that at Bombay the maximum pressure precedes nearly by a month the minimum temperature, while the minimum pressure is a month later than the maximum temperature. This is also true for all the stations in North India. At Madras, however, and Trevandrum, January becomes the month of maximum pressure. I do not, therefore, place much weight on this fact as showing that the two oscillations are not cause and effect. The month of maximum pressure at Pekin agrees most nearly with that of minimum temperature.

I have stated in the first article on this subject that I did not admit that the oscillation of pressure was due to that of temperature, and therefore could not allow that a higher annual mean temperature [would in any case cause a lower annual mean pressure] From the fact that the annual variation of pressure and temperature in Central Asia is greater than in any other portion of the globe, the greatest pressure coinciding nearly with the lowest temperature, and the least pressure with the highest temperature, it was concluded by Mr. Chambers that years of greatest mean pressure should also be years of least mean temperature. Now if we assume that the pressure depends only on the mass of the air and watery vapour in it, as the former is constant, and the latter, the only variable part, is greatest when the temperature is highest, it would follow that years of greatest heat should be years of greatest pressure, which is just the reverse of the conclusion deduced by the analogy from the annual variations.

Indeed, it is one of the great difficulties in the hypotheses which have been proposed, to explain the annual variation of pressure of the mixed atmosphere, that when we subduct the vapour pressure, as far as our means of calculating this exist, we have a much larger dry air oscillation than before.

I gave, however, different reasons for concluding that the range of temperature was not itself the cause of the diminished pressure, although the two go nearly together. One was that the observations of Bombay showed the greatest pressure to precede the lowest temperature by a month, and this is true for all the stations in the groups of North India already given. I also pointed out that were the two directly related, the mean pressure at Trevandrum should be greater than at Pallamcottah by nearly one-tenth of an inch, which is not the case, the isobars and isotherms having no relation to each other.

If we suppose that we have the same atmosphere over each station as over the whole earth, there is no possibility of explaining the variation of pressure by that of temperature. The only known property of heat which affects the mass has no doubt been employed to cause the hotter air to flow away somewhere, and surely in that case it should flow to the nearest colder station, where the pressure is less; but we have seen that this is not so in the case of Trevandrum and Pallamcottah, nor is it so in the valleys of the Ganges and Indus, where the oscillation increases as we ascend from the sea. These oscillations proceed with the greatest regularity, approximately in proportion to the temperature variation from month to month, and without the slightest regard to the hypothesis which should cause equilibrium in twenty-four hours, by the sliding of the most expanded masses over those least so. In what way, then, can we associate the two oscillations if one is not the cause of the other? I have long ago suggested that the varying humidity of the air may be in question; this is only a suggestion. I do not mean the mere tension of vapour—as already stated when we try to get rid of that from the total atmospheric pressure, the subject becomes more difficult, the dry air oscillation being greater not less than that of the whole—but if we suppose that the attraction of gravity is not the only attraction which affects the pressure of the atmosphere, but that this pressure varies through some other attracting force such as an electric attraction of the sun depending upon the varying humidity of the air, and this again depending on its temperature; we should find a method of relating the two variations which does not exist if gravitation alone is employed.

It is quite certain that many physicists will not admit the idea of an electric attraction on our atmosphere in the present state of our knowledge, hence the efforts to make expansion, and a shuffling of the atmospheric strata suffice. We must not, however, in our ignorance, attempt to force conclusions in opposition to facts, and if these can be satisfied more easily and with greater probabilities in its favour by the aid of the hypothesis of an electric attraction of the sun, that hypothesis will have a better claim to acceptation than the other.

I shall here note a few facts which cannot be explained by thermic actions.

1. I have shown that on the average of many years observation in our latitudes the mean pressure diminishes at the rate of 0·038 inch of mercury for every one hundred geographical miles we proceed towards the north. This has been called a gradient from the similar term used in railway slopes; but it is no slope, it is a level of a surface of equilibrium like that of the sea. It is the mean heights of the barometer at the sea-level which indicate the form, if we may say so, of the equilibrating atmosphere.

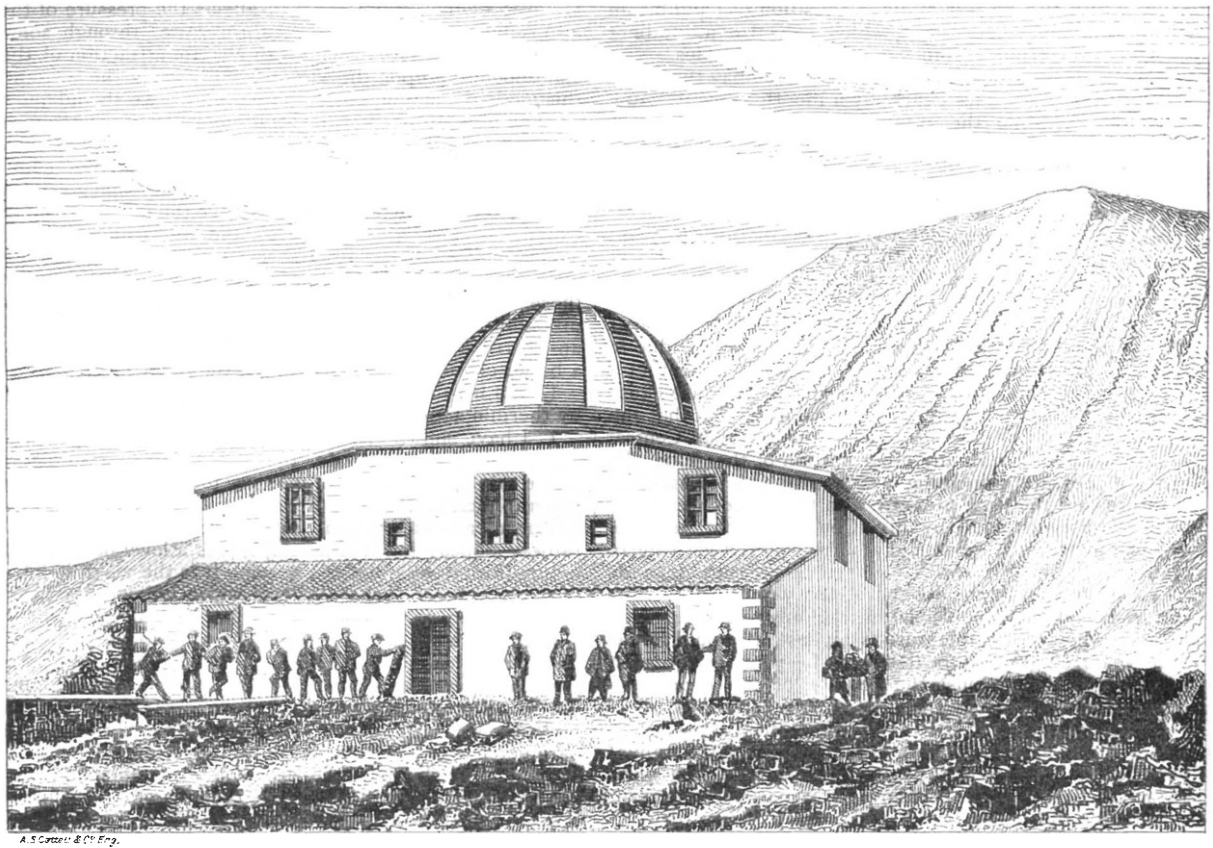
2. In India we have seen that the atmospheric pressure oscillates at each station even when these are quite near to each other, independently of the known laws of equilibrium of pressure of gases.

When we turn to the semi-diurnal oscillation of the barometer we are only amused at the attempts made to explain it by shuffling the atmospheric strata. Nothing can be more certain than that the theories of expansion, or resistance to expansion and overflow, are the vain efforts to make the laws of nature agree with a theory. Over the great ocean within the tropics, where the diurnal variations of temperature are small and the air is absolutely without perceptible currents for days together, the barometer rises and falls a tenth of an inch twice in twenty-four hours with the regularity of the solar clock. The action of the sun on the whole atmosphere which produces this movement varies chiefly during the day hours at inland stations with the temperature oscillation, so that, as in the case of the annual variation, the fall of the barometer at 4 P.M. is greater in the same latitude as the temperature is higher. This variation occurs during the most complete calms; the smoke rises vertically from the plains of Tinnevelly; no current is visible in the motion of the clouds; yet the barometer falls at four in the afternoon as it did at four in the morning, only it falls farther.

THE ETNA OBSERVATORY

THE accompanying illustration of the Observatory on Mount Etna is reproduced from the *Memoirs* of the Italian Spectroscopic Society. It shows that the building is so far complete, and surmounted by its revolving dome for the protection of the large Merz equatorial of thirty-five centimetres aperture. In the engraving the volcanic cone appears much nearer the Observatory than it really is. The work of building was suspended during the stormy weather of 1879, but was completed in the summer of last year. But it cannot be said that the

building is yet quite finished and ready for occupation ; a good deal of work has yet to be done to the internal walls, the doors and windows, flooring, &c., besides the scientific equipment of the building. Therefore the announcement by the Alpine Club of Catania that the building would be ready for inauguration at the meeting of the Alpine Congress in Catania next September was premature. The Observatory will not really be ready to be opened till 1882. The difficulties that have had to be contended with can only be comprehended by those



who have visited the place ; all the materials have to be conveyed 3000 metres above the level of the sea, and that during only three months in the year. So that even if not ready till 1882, the work may be said to have been rapidly accomplished. The mounting of the equatorial is finished and the construction of the meteorological apparatus is being proceeded with. The Ministers of Agriculture and Public Instruction are doing their best to provide the Observatory, by 1882, with a director and a staff both of astronomers and meteorologists.

MODE OF MASKING OR CUTTING OFF SHARPLY THE LIGHT FROM REVOLVING APPARATUS ON ANY DESIRED COMPASS-BEARING BY MEANS OF A RECIPROCATING SCREEN

OWING to the optical properties of the lens employed in revolving lights, a formidable element of difficulty comes in the way of effecting a sharp cut-off on a particular bearing; for the direction of the axis of the beam of light which is projected by the lens is being continually changed in the horizontal plane by the revolution of the frame on which it is fixed. So long as the axis of this beam of rays points outside of the line of obscuration the light will not of course encroach on the danger arc, unless to a small extent, when the axis is nearly on the line of cut-off due to the ex-focal rays proceeding from the outer edges of the flame. The light however will begin to be diminished in power from a bearing varying from 12° to 21° outside of the line of cut-off dependent on the size of the lens, the light on the line of cut-off being diminished to the power of one-half. But when the axis crosses that line, then as the rays which come from that part of the apparatus which is still outside of the darkened panes of the lantern is not intercepted by them, the light will begin to be seen within the arc of danger, and as the apparatus goes on revolving the axis will at last point from about 12° to 21° within the danger-arc, according to the breadth of the lens which is employed. Owing to this peculiarity of a revolving light the difficulty of confining the flashes within any required arc of the horizon by means of *fixed* screens is in fact an insurmountable one.

The mode which I have to suggest is as follows :—In front of the revolving apparatus and on the safety side of the danger arc, let a light canvas or metallic screen be constructed for running on rollers on a slightly inclined rail or circular path close to the apparatus. If now a small projecting rod or snug be fixed to the side of each lens it will in revolving be brought against the edge of the screen, and will gradually press the screen before it up the inclined plane at the same rate of motion backwards